[15] The measurement of the multiplicity of an object is formally equivalent to [1], the counting of objects. Again there is no way to select a standard limiting magnitude (or limiting resolution) and usually the multiplicity is expressed by giving the number of entities as a function of some limiting factor that controls their detectability (such as the magnitude). It is only when the multiplicity converges to a finite value as the other factor is extended indefinitely that a stable primary value of multiplicity can be given.

[16, 17] These measurements can represent either (*a*) the displacement of the object from some mean or standard position, or (*b*) the measurement of components of the object from some fiducial position. The primary co-ordinates would be the radial distance (usually in seconds of arc from the central fiducial position represented by [2, 3]), and the orientation (anticlockwise from N) at epoch 1900·0.

[18] The practice of expressing surface brightness as an apparent magnitude of a defined area has the advantage that great differences of brightness can be expressed without change of units. This being so it is desirable that the unit of area be selected once for all, and the square second of arc would appear to be the suitable choice for primary measurement. However, when components of radiation have to be added the logarithmic magnitude scale is inconvenient and one might adopt as a second choice the number of $m_v = 0$ or $m_v = 10$ stars per square degree.

[19] The measurement of number density of entities or components of an object is similar to the measurement of its multiplicity [15].

[20] The custom of expressing wavelengths shorter than λ 2000 in vacuum units and those longer than λ 2000 normal dry air at 15°C is likely to be continued. In the radio spectrum frequencies are more usually adopted for accurate work and the ambiguity avoided.

[21] No ambiguity is involved in expressing wavelength shifts in Ångstroms or kaysers (cm^{-1}).

[22] The intensity of an emission or absorption line that is associated with a stable continuous spectrum may be expressed as an equivalent breadth of that continuum. This is convenient both for measurement and analysis. However, if no continuum is present, measurements must be given directly in physical units, either absolute or relative. For the sake of uniformity it would be preferable to express primary absorption line intensities as equivalent breadths, and emission intensities in physical units such as erg $cm^{-2}s^{-1}$.

[23] Line broadening is usually symmetrical and regular and hence its measurement does not involve the difficulties of [14], which is analogous. The accepted primary measurement is the total wavelength distance between the points where the intensity is half the maximum.

[24] The measurement of line multiplicity in astronomical sources presents no difficulty provided the spectrographic resolution is finer than the line width. Components that would have been resolved if the line width were much smaller are not to be regarded as adding to the multiplicity.

[25] Absolute measurements of spectral intensity f_λ may readily be expressed in physical units such as erg $cm^{-2}s^{-1}Å^{-1}$. However, actual measurements are usually relative and it is recommended that these should be expressed in the same physical units relative to the value at 5400 Å. Such measurements could be readily related to visual magnitudes.

Reviewing this discussion we find that standard practice provides suitable primary measurements in most cases. A new measurement to represent the population of objects seen from the earth is suggested in [1], and a suggestion for standardizing the size of nebulae, *etc*., is revived in [8, 9]. On the other hand, we have found no suitable primary measurement of the duration of irregular events.

If astronomers in the course of their many and varied observations could add as many as possible of the above primary measurements to their programme the results would always be welcome. In particular, if the work has other intentions the primary measurements should be added as a by-product. There are, of course, many other primary factors (such as distance) which are also needed from observers and which fall outside the scheme of observations considered here. But the same arguments apply—if a clear-cut, unambiguous, and well-accepted method of expressing the factor is available there will be much better prospects of obtaining usable results.

Fact and Inference in Theory and in Observation

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Summary

A critical examination is made of the relative reliabilities of theoretical and observational work in astronomy. It is found that, contrary to widely held opinions, observational papers are no less liable than theoretical papers to reach erroneous conclusions.

The aim of the present paper is an examination of the relative reliabilities of so-called "theory" and so-called "observation" in astronomy. They may simply be regarded as different tools used by research workers. Such examinations of relative reliabilities are common enough in all fields of science. Measurements made by different instruments are generally found not to agree amongst themselves, and the subject of the combination of observations attempts to deal rationally with such difficulties. Statistical weights are used to describe the relative reliabilities of similar instruments. In the event of a contradiction between two instruments it is helpful to have some knowledge of the degree of certainty to be assigned to them. In ordinary laboratory practice this assessment is generally based chiefly on past experience with the instruments concerned, combined with comparisons of their constructions and estimates of how "direct" the measurements are.

This purely empirical approach has been found far more satisfactory in laboratory work than reliance on emotional prejudices, however real they may seem without critical examination. Empiricism is vital to all scientific progress, and there seems to be good reason to apply this empirical approach, which has been so successful in the laboratory, to a rather wider field.

It is fashionable nowadays to divide astronomical research into two categories, "theoretical" and "observational". These terms are ill-defined and lack sure philosophical significance, but it is nevertheless true that most astronomers would have

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little doubt or disagreement on how to classify most published papers. All references in this essay to "theory" and to "observation" are intended only in this colloquial sense and should not be understood as more than a rough and ready classification.

There is undoubtedly a widespread opinion that astronomical theory is, by and large, airy speculation, that it changes from day to day in its most fundamental tenets, that it is based on ill-considered hypotheses and that its reliability should accordingly be assessed to be low. By contrast the results of observational work are, according to this opinion, solid incontrovertible facts, permanent and precise achievements, that will never change and whose reliability is accordingly high.

In my view these opinions are unfounded and false, and their prevalence does great harm to the progress of astronomy. If they were not injurious, there would be little point in attempting to dispel these views, but, in the present state of astronomy, conflicts between observation and theoretical results continually arise. In the past, credence in such a conflict has generally been given to the observational result, and theoretical advances of considerably significance have been held up by undue reliance on the observations. The chief purpose of this paper is to show that in such a conflict it is far wiser to keep an open mind or even to lean to the side of the theory. Such an attitude, which can be firmly based on a critical examination of past experience, is likely to be far more helpful to the progress of astronomy than reliance on prejudices based on the emotional significance of such outworn phrases as "observational fact" and "theoretical speculation".

Before an examination of the relative reliabilities of these so-called theoretical and observational results can be attempted, it is desirable to show that theoretical work falls into two quite distinct classes. One is essentially an attempt to classify observational material; the *ad hoc* assumption is made that the type of classification is based on intrinsic qualities without making these particularly clear or (and this is the chief characteristic of this type of structure) linking up these qualities with known phenomena. An example of this type of structure is the old RUSSELL "theory" of the luminosity-type diagram. As will be remembered the principal suggestion of this theory was that the different appearance of giant and dwarf stars was due to the fact that they were made of different materials, "giant stuff" and "dwarf stuff" as the terms were. Any further explanation beyond this, any link with the known properties of material was left for the future. As is well known the suggestion was later disproved, but it is a typical example of such suggestions.

An example of more recent date concerns the ingenious observation by McKELLAR (1949) of the abundances of carbon isotopes on certain very red giant stars. He suggested that the stars in which the isotope ratio agreed with that produced in the Bethe cycle were well mixed stars, and those where the ratio was different, were not mixed. The sole purpose of any such suggestion can only be merely to fit the immediate observational classification. Indeed it is little more than a re-wording. To anyone who has the slightest knowledge of stellar structure, McKELLAR's suggestion can only sound utterly unconvincing, since it raises many more questions than it solves.

An entirely different type of logical structure to which frequently the same name of "theory" is given, is in fact a development of terrestrial physics. An attempt is made to apply the laws of physics as obtained in the laboratory to the conditions of the astronomical problem in question. Naturally it is sometimes necessary in the present state of knowledge to make a number of additional assumptions and the value

of the attempt will depend greatly on the clarity and explicit nature of these assumptions. In my view misunderstandings have frequently arisen through confusion between the two types of approach both of which have been referred to as "theory". Different names are required, and I suggest "observational inference" for the first type of structure, and "physical theory" for the second type. An example of this second type of structure is the theory of stellar constitution.

In attempting to assess the relative reliabilities of theoretical and observational work in the past it is necessary to establish a criterion of failure. This is not quite as simple as one would expect it to be.

It is an essential feature of science that everything in it may stand in need of revision as a result of new and unexpected evidence—that it contains no "ultimate truths". Such truths are certainly outside the scope of science, though they may appertain to philosophy and to religion. Whenever we examine any approach to science we will not be surprised to find that changes occasionally occur, and this is true both of theory and of observation. Some such changes arise indeed merely from a widening of the field of enquiry. Classical mechanics was found wanting when it was applied to atomic dimensions and had to be replaced by quantum mechanics. The validity of classical mechanics in the macroscopic field is, however, not affected by this change. The changes that are to be examined here, the errors that should be taken account of, are of a far grosser and less subtle kind. They are simply statements that were later shown to be incorrect in the very field to which they were originally intended to apply within the accuracy originally claimed. Such errors have almost always been a hindrance rather than a help to the progress of science and we shall understand by the word error this its usual conventional meaning (as, for example, in the expression "probable error") without in any way entering upon the metascientific problem of a definition of the term.

Three questions seem then to arise:

(i) Is observational knowledge by its very nature more or less certain than theoretical knowledge?

(ii) As an empirical test, have theories in the past been shown to be in error more or less frequently than observational results?

(iii) Is an error in observation likely to persist for longer or shorter than an error in a theory?

Where the first of these is concerned the extreme complexity of present-day observational methods hardly need be stressed. To derive any significant astronomical result from the blackening of a photographic plate or the simple reading of a meter a tremendous amount of intervening work has to be done. Corrections may have to be applied, calculations and reductions may have to be carried out, and above all interpretations requiring a great deal of theoretical background may have to be made. Consider, for example, such an apparently straightforward matter as the determination of the masses of an eclipsing binary. Gravitational theory, tidal theory, theoretical reflection factors, and other theoretical notions have all to be brought in and used, frequently to the limits of their power. Or consider work on spectroscopic abundances where not only the most accurate photometry, but also particularly complex aspects of quantum theory are involved. Nobody can fail to admire such work, but by no stretch of imagination can it be termed purely factual or purely observational. And yet this is what some people seek to do!

In a recent review of Mr. HOYLE's broadcast talks by Dr. WILLIAMSON (1951), a review possibly written more in anger than in earnest, the reviewer states that he has attempted to establish the percentage of astronomical "facts" as compared with "theory" in Mr. HOYLE's talks. But what is an astronomical fact? At most it is a smudge on a photographic plate! Does he expect Mr. HOYLE to give a broadcast talk on smudges?

The purely factual part of the vast majority of observational papers is small. It is also important to realise that these basic facts are frequently obtained at the very limit of the power of the instruments used, and hence are of considerable uncertainty. To refer to observational results as "facts" is an insult to the labours of the observer, a mistaken attempt to discredit theorists, a disservice to astronomy in general and exhibits a complete lack of critical sense. Indeed I would go so far as to say that this sort of irresponsible misuse of terminology is the curse of modern astronomy.

Present-day observational astronomy may fairly be called the science of extracting the maximum information from the fundamentally meagre data that can be obtained about outer space, an endeavour to stretch both observation and interpretation to the very limit.

A similar examination of physical theories in astronomy reveals that their primary basis is very sound indeed, since it rests on established terrestrial physics. But in order to apply this knowledge to astronomy inferences of considerable range have to be made, sometimes with the aid of additional assumptions. In the theory of stellar structure, for example, results obtained in thermodynamic systems of temperatures of at most 4000° or in non-thermodynamic systems (particle accelerators) employing extremely tenuous matter have to be applied to dense matter at millions of degrees. It is only because of the comprehensive nature of laboratory physics that such extrapolations are possible. Other examples of a similar nature could be given, but it would probably be fair to sum up the situation by saying that in *observational work long chains of inferences are based on frequently somewhat uncertain data, whereas in physical theories of astronomy, though long chains of inferences are also used, they are generally based on much more reliable experimental data.* There is, therefore, no reason to expect any marked difference in the degrees of reliability of so-called theory and of observation, unless indeed it is that theoretical results are of greater reliability.

We can now turn our attention to the second question, that of empirical test. The question is, have theories been disproved more or less frequently than observational results? Clearly it is difficult to give an exhaustive list on either side. On the theoretical side I might mention JEANS' proof (1925, 1927) that stars would be unstable if the subatomic generation of energy depended very sensitively on temperature, a proof that confused the development of the theory of the constitution of the stars until COWLING (1934, 1935) showed it to be fallacious. Again JEANS' "long time scale" for the age of the galaxy (1929) was widely accepted until it was disproved by many arguments (see BOK, 1946). Finally, one might mention EDDINGTON's estimate of the hydrogen content of the stars as about 35 per cent (1930). This last error must be shared between theoretical and observational astronomy, since spectroscopists were of the same opinion. The recognition that the hydrogen content was far higher came more recently (DUNHAM, 1939; HOYLE, 1947).

Although in my own work I am more in contact with theoretical than with observational research, yet I find that I have more often met observational errors. One might recall VAN MAANEN's observations of the proper motions in extragalactic nebulae (1916, 1921, 1922, 1923, 1925, 1927), observations that have been proved to be incorrect not by factors of 2 or 3, but factors of 100 or 1000 (HUBBLE, 1935). Then one might mention ADAMS' "discovery" of the EINSTEIN shift of the spectral lines of Sirius B (1925). This shift is now not only considered to be practically impossible to measure, but is known through modern quantum theory to be very seriously obscured by almost incalculable pressure shifts (LINDHOLM, 1941; ADAM, 1948). But the shift "observed" in 1925 was supposed to agree as well as could be expected with relativity theory without account being taken of these pressure shifts.

Yet another example in this field is formulated by the Trumpler stars (TRUMPLER, 1935; An unjustified interpretation of an uncertain and difficult observation was widely accepted as an established fact, as a proof that extremely massive stars existed with known luminosities and radii that were apparently in contradiction with the theory of stellar structure. Confusion was caused by reliance on this result, but now it is regarded as having been based on erroneous interpretations (STRUVE, 1950).

In an earlier period, astronomers in this country, after failing to discover Neptune, endowed this planet with a ring and satellites (LASSELL, 1847). More recently, HUBBLE and HUMASON (1931) inferred from their data that the constant of the red-shift of the nebulae was $4{\cdot}967 \pm 0{\cdot}012$ and soon afterwards, from almost the same data, that it was $4{\cdot}707 \pm 0{\cdot}016$ (1931, 1934; HUBBLE, 1936). Similarly the last determination of the solar parallax (SPENCER JONES, 1941) is well outside the three-fold stated probable errors of earlier determinations which vary amongst themselves far more than their individual stated errors.

In a field with which I have been in close contact, Struve's recent work on Capella (STRUVE, 1951) has come as something of a shock. We were told in the most meticulous survey (KUIPER, 1938) that the masses of the components of Capella were the second best known ones, that, except for two cases, all other mass determinations were at least twice as uncertain as Capella, and, except for four other cases, more than three times as uncertain. And now it appears that even in the case of Capella there has been all along an error of more than 25 per cent. EDDINGTON in particular used Capella as his standard when he developed the theory of stellar structure. More recently, but for STRUVE's timely discovery of the mistake, the subject of the structure of Red Giant stars would have been put on the wrong track by reliance on this supposedly so precise and permanent fact.

These examples, though by no means exhaustive, will illustrate sufficiently the thesis of this essay. It seems that, by the empirical test, errors in theories are if anything less frequent than in observational work. A detailed numerical test is difficult, but what I have said is enough to refute the view that theories are airy pieces of guesswork disproved every few days whereas observational results are "hard", "incontrovertible" facts.

We come then to the third question: are errors likely to persist for longer in theory or in observation? The answer to this question is clear. Observational equipment is so scarce, is devoted to so many tasks, and is so difficult to set up, that repetitions and checking are not as common as one would hope, particularly in the case of many theoretically important measurements. There is also a widespread opinion, an opinion that I hope will not persist, that the certainty of observational work is so great that no repetitions are required. This uncritical attitude is greatly to be deplored. If more effort were devoted to repeating observations, the gain in certainty would be of great value.

The attitude and climate of opinion in which theoretical astronomy is conducted are entirely different. Almost every paper is received with sceptical interest and many papers immediately stimulate work connected with their proof or disproof. While in observational work it is unfortunately considered somewhat impolite for one observer to criticize the observations and immediate inferences of another observer, similar criticism between theorists is luckily considered perfectly natural. There is therefore a considerable likelihood of an error being rectified speedily. As a recent example of this I might mention a paper by RICHARDSON and SCHWARZSCHILD (1948) & SCHWARZSCHILD (1948) on Red Giants which was refuted by GOLD (1949), his paper being submitted less than two months after the publication of the first paper. An error in a paper on stellar structure by CHANDRASEKHAR and SHOENBERG (1942) was shown up speedily by HOYLE and LYTTLETON (1946). Of course, sometimes the rectification of an error takes much longer. JEANS' statement (1925) about the instability of stars with temperature sensitive energy production was finally disbelieved only after COWLING'S work in 1934, twelve years after the claim had appeared. But even this interval is not long compared withthe cases quoted in observational astronomy where the intervals are usually more like twenty years. The intricate nature of observational equipment makes intervals of several years almost unavoidable, but the great lengths of interval occurring are probably due to the unfounded prejudice of regarding many observational results as facts not requiring confirmation.

The persistence of observational errors being generally much greater than of theoretical errors implies that if the average fraction of errors produced in the two branches is roughly equal then the fraction of incorrect current observational work is considerably greater than the corresponding fraction for theoretical work. Careful statistical analysis would be required to confirm this statement and put it into quantitative form, but the arguments given here seem to support this conclusion strongly.

So far attention has been confined to physical theories rather than observational inferences, since there is a considerable structural difference between them from the point of view of scientific methodology. The fate of observational inferences is not encouraging to anyone thinking of relying on this method, many of them having turned out to be quite incorrect. Nevertheless they still continue to be made, mainly, though not entirely, in the context of observational papers. This seems to be the result of a deep human prejudice, that if only one continues to look at an object for long enough its nature will become apparent. In science this is, of course, nonsensical. One could stare at a piece of wood for years if not generations without discovering its atomic nature, or being able to infer its properties in any way from appearances. I must refer again to Dr. WILLIAMSON'S article (1951) which is so usefully revealing in presenting current prejudices without any attempt at veiling or rationalizing them. He clearly considers it to be a valid argument that people who have never done actual observational research are not entitled to discuss astronomy. A more

preposterous statement is hard to imagine. It is on the same plane as the statement that only plumbers and milkmen have the right to pronounce on questions of hydrodynamics.

As an example of an observational inference one can quote from STRUVE'S book (1950, p. 116): "It looks as though the K-type and M-type dwarfs represent something in the nature of the final stage. . . . This part of the H-R diagram resembles a sink into which many stars drop . . .".

This is not the place for discussing such an idea in detail but I cannot help suspecting that the feeling described is at least partly due to a prolonged study of HERTZSPRUNG-RUSSELL diagrams drawn in the conventional way with the red dwarfs *near the bottom* of the picture.

The final point I wish to make concerns stated probable errors. All too often subsequent work has shown that they bear little relation to the actual errors made but are at most an indication of the internal consistency of the methods used. It would be a tremendous help if more observational papers were to contain (as some do now) a reasonable assessment of the errors that may have arisen. On the theoretical side it would be similarly of great advantage if more papers could contain clear explicit statements of the assumptions made, of every appeal to observation, and of every subsidiary hypothesis. Then the observational disproof of a theory would convey immediately valuable information. It would become clear that one of the bases of the theory was wrong, and such discoveries have frequently been very valuable, as for example, when the MICHELSON-MORLEY experiment showed that the velocity addition formula underlying the original theory was wrong. If these habits of clearly stating uncertainties and assumptions became general, and if prejudices regarding observational results as facts and theories as bubbles were overcome, astronomy would greatly benefit. Both so-called theory and so-called observation are liable to error, and critical appreciation and impartial scepticism are the best foundations for progress.

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Philosophical Aspects of Cosmology

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1. THE purpose of this paper is to indicate, but not to solve, certain philosophical problems peculiar to the study of cosmology. Cosmology and cosmogony—no attempt is made here to distinguish them, and they are in fact inseparable—are at the present time primarily scientific subjects; that is to say, we are no longer forced to approach them on grounds of pure reason alone but have in our possession a growing body of observed facts which must form the data on which our reason begins to operate. Nevertheless, they have the peculiarity that in them we treat the whole field of investigation as having characteristics of its own, independent of the characteristics of any of its parts, and it is those universal characteristics that we seek to discover. But, up to the present at least, the part of the universe that we can observe is at most only a very small portion of what we have reason to believe exists. We have therefore to introduce considerations over and above the ordinary scientific process of inductive generalization, and those considerations are philosophical in character and so give to cosmology a philosophical aspect which the other departments of science do not show in the same degree.

An example will make the point clearer. When NEWTON demonstrated that within the solar system the movements of bodies everywhere conformed to the law that every piece of matter attracted every other piece of matter with a force varying directly as the masses of the bodies and inversely as the square of the distance between them, this law was generalized to apply to matter everywhere, and so became known as a *universal* law. But it was not a law of the universe; it was a law that, supposing it to be true, was exemplified wholly and completely in every part of the universe, but it had nothing to do with the universe as a whole. The universe might be large or small, finite or infinite, eternal or temporary, homogeneous or heterogeneous—in fact, it might, as a whole, have any conceivable characteristics at all, and the Newtonian law of gravitation would be the same in all cases. Similarly, the laws of thermodynamics are universal laws but not laws of the universe. Again assuming them to be true, they characterize any closed system whatever, small or large, irrespective of whatever else the universe might contain; we therefore learn

from them nothing at all about the character of the universe *as a whole*. On the other hand, when you say that space has a positive or a negative or a zero curvature, you are stating a law of the universe, but you are saying nothing at all about any particular part of the universe. The curvature in a given region may be anything; it is only the universe *as a whole* that is supposed to have the curvature you postulate. Accordingly, the assertion that the universe has this curvature must rest on something other than direct generalization from observation or experiment in limited regions; it must involve reasoning over and above the ordinary type of scientific reasoning. It is the problems connected with that kind of reasoning that are most properly called philosophical problems of cosmology.

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2. By its very definition the universe is necessarily unique. The first question that arises, therefore, is whether its uniqueness is, so to speak, essential or accidental. That is to say, can we properly imagine different kinds of universes that might have been formed, and so present ourselves with the problem of showing why it was in fact the actual one that came into existence; or must we take it as a primary axiom that any conceivable alternative to the actual universe must necessarily be impossible? This question must be answered before we can decide whether or not familiar scientific methods of attack are legitimate when applied to the universe as a whole. For example, in the statistical mechanics of GIBBS, when we wish to study the behaviour of a sample of gas, we first of all suppose the sample to be composed of a very large number of molecules, each behaving in an unspecified—or at most an incompletely specified—manner, and then consider a very large number of such samples. Owing to the incomplete specification we cannot deduce with certainty how this *ensemble* of samples will behave, but we can deduce its probable behaviour. We then draw conclusions, from this probable behaviour of the *ensemble*, about the actual behaviour of the sample in which we are interested.

this is a nonsensical question

Now whatever philosophical doubts may be aroused as to the validity of deductions made in this way, there can be no doubt concerning the possibility of making the investigation. Undoubtedly a very large number of samples of gas of the kind postulated do exist, and undoubtedly, if they are composed of molecules, we must leave unspecified the instantaneous positions and momenta of those molecules and therefore have no right to assume that such positions and momenta in the various samples bear any relation to one another. But have we any right to apply the same kind of treatment to the universe? Can we consider an *ensemble* of universes, in each of which the stars and nebulae can form any configuration whatever, and then draw conclusions about our actual universe from the probability of behaviour of this imaginary *ensemble*? It is at least a plausible proposition that we have no right to talk of the probability of an event before we are satisfied that the event is possible. We have, so far as I know, no assurance that any universe other than one *is* possible.

3. Consider now another example—not an imaginary one, but one that has revealed itself in the recent history of cosmology without, apparently, arousing the philosophical questions that should have been asked. The notion of "stability of equilibrium" is a very familiar one in mechanics. A system is said to be in *equilibrium* when, in the absence of disturbances from the outside, its state remains unchanged for an indefinite time; the equilibrium is said to be *stable* when, if the system is momentarily disturbed by an indefinitely small external impulse, it automatically